

Letters to the Editor

TWO CORRECTIONS

Sir:

It has recently come to my attention that there is a pair of errors in my article, "The Political Context of Economic Adaptation in the Southern Highlands of Tanganyika" (AA 67:435-441). On page 439 of that article I gave the date 1899 for the death of Mkwawa, perhaps the most famous *Mtwa* of the Hehe. This is an unfortunate repetition of a curious error made by the usually reliable German administrator and ethnographer, Ernst Nigmann (*Die Wahehe*, Berlin, 1908, p. 19). This date is even given on the headstone of Mkwawa's grave at Kalenga, Uhehe. The proper date should be 19 July 1898, as contained in the report of *Feldwebel* Merkl published in the *Deutsches Kolonialblatt* of that year (*D.K.B.*, 1898, pp. 645-646).

The second error is a glaring typographic reversal of two numbers, and it is something of a surprise to me that no eagle-eyed readers with an interest in demography have written about it. On page 439 of my article, the Tanganyika Census of 1948 is cited as listing the population of Iringa District as 108,906 (E.A. Statistical Dept., *African Population of Tanganyika Territory*, 1950, p. 64). This is an inversion and should read 180,906. The proper figure makes my remark that there is evidence of rapid population growth seem somewhat less of a startling understatement, for the census of 1957 gives the District total as 245,965, which according to the erroneous total for 1948 would represent a growth of 137,059, or more than a doubling in just ten years! (E.A. Statistical Dept., *General African Census, August 1957. Tribal Analysis*, Part I, 1958, p. 54). The true growth of 65,059 is high enough without the augmentation of this misprint.

I hope that these errors have not caused inconvenience to other workers, and I am sorry that I failed to notice them as soon as they were published. I must thank Dr. Alison Redmayne, who is currently engaged in research on this area, for her enquiries on these two figures.

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L'AFFAIRE CAMELOT

Sir:

L'affaire Camelot lives again in Dale Johnson's recent letter (AA 68:1016-1017) and in Henry Raymont's articles (November 18 & 21, 1966) in the *New York Times*. I can sympathize with Johnson and his fears about future research abroad, but I must say that his information about Camelot and the supposedly connected projects, Simpatico and

Task, is inaccurate. Neither research project was related to Camelot; both preceded the first scratchings that turned into Camelot; both had origins that were not at all related to the famous debacle known by the musical comedy name. In fact, Task probably refers to a research study that was my responsibility and concerned the study of migrations of Andean *campesinos* to the Amazonian jungle, where they were establishing farms and homes—a most uncynical and unsinister project. In the jargon of SORO (Special Operations Research Office, The American University), "task" merely meant project. The ranking members of SORO assumed the highly important job of inventing acronyms for all the research projects and had some mystical, unerring aim in getting so many feet in their mouths. Cases in point: (1) "Camelot," with its memories of virtuous knights rescuing fair maidens from the jaws of fiery dragons; (2) when "Camelot" had to be discarded, its staff was renamed "Social Analysis Group," with the acronym "SAG"; (3) when it was found that "SORO" now caused raised eyebrows and stirred thoughts about special operations and "Camelot," they searched for a new name for the organization and have found "CRESS" (Center for Research in Social Systems), with its associations of cavities, aches, and even anemic ladylike sandwiches. I am attempting to demonstrate how these acronyms were born. My project was dubbed "Colony" by my superiors, who then worried about its negative connotations (although "colonia" and "colonization" are used in Spanish in a contemporary and positive sense) and changed the name of my project to "Resettle" (like what comes after nausea, Pepto-Bismol, sodium bicarbonate...).

Henry Raymont's coverage of Professor Beal's report to the American Anthropological Association's annual meeting in Pittsburgh points out the entire profession's concern and fear that opportunities for foreign study and research have been and will be further limited by Camelots and Camelot-like arrangements between the federal government and researchers and scholars.

In spite of all the words spoken and written about Project Camelot, it has not thus far been objectively discussed and evaluated. The press coverage has been inaccurate and spotty; the apologetics have been far from honest; the critiques have been shrill.

As an anthropologist who was until recently a senior researcher of the American University's Special Operations Research Office—but who was not connected with Project Camelot—who observed the machinations of Camelot as it developed and as it dried up, and who was carrying out field research in a South American country with the knowledge and support of that country's government, I would

like to shed some light on this episode, which seems to be splitting the university community from the federal government and slurring certain researchers who have worked under government auspices.

Simply put, the Camelot project turned into a disaster for a number of related reasons. For the sake of brevity, I would like to list them:

(1) Federal research funds and programs—particularly in the social science fields—are now controlled by bureaucrats who once had some claim to academic credentials and competence. In the Department of Defense, the psychologists—who have demonstrated their worth in the development of selection tests and in human engineering—have been put in charge of programs and projects that require the contributions of historians, political scientists, economists, area experts, anthropologists, and sociologists. Because the psychologists find themselves in these strange academic waters, they are forced to adopt the chameleon-like qualities of the old-line bureaucrat and retreat into organization charts, constant reorganizations, programming, funding, and the like. This happens in the Washington environment for two reasons: (a) the psychologist-administrator has a minor substantive contribution to make to these social science programs, and (b) he quickly becomes aware of the power of funding and organization and fights like a wounded animal to retain it. I am describing bureaucrats both in government and in research institutions funded by government.

I sincerely hope that my remarks will not be interpreted incorrectly; I am not mouthing anti-psychologist sentiments. In fact, I admire the discipline tremendously and consider it part of my own training. I am making a distinction between the competent and cautious psychologists who work at their trades and the new-bureaucrats—former-psychologists who are in seats of power in Washington. I would make a similar distinction between good and poor types in all disciplines.

(2) Social science programs that call for field work in foreign countries (e.g., Camelot) are greatly disadvantaged because the individuals in charge of the programs have never worked abroad. Their field experience—and we are most generous in calling it field work—has most frequently concerned captive samples of U.S. soldiers and, perhaps, college sophomores. Such circumstances hardly prepare one to consider the delicate matters of the national sentiments of Chileans or Brazilians, of the relationships between hosts and guests, of needed reciprocities in such binational relationships. Of course, I have not touched on the critical matter of rapport (between the American researcher and his foreign informants) that must be developed if the data obtained are to be meaningful, valid, and reliable. Some critics of Camelot have attacked it as cultural imperialism. I would prefer to describe what has happened as intercultural ignorance or naïveté.

(3) Let's put aside the scientific worth of Camelot

for the moment and discuss the market place. Certain university types were blinded by the size of the budget and became consultants. The staffers who composed the research plan glutted the document with military jargon to please the military sponsor, and spoke to some members of the academic community about the project's pure research intent and its eventual open publication. Hypocrisy was part of Camelot as well as naïveté.

(4) Apparently, no individual in a decision-making or research position firmly and formally reached out to discuss the political realities of conducting a project of such magnitude in various foreign countries with the desk officers in the State Department or with our embassies in these countries. Admittedly, the State Department had assigned a man to attend some of the Camelot planning conferences, but it seems that "he only audited the course." The State Department has claimed that it was never formally briefed on Project Camelot. How do we explain this—hypocrisy or interagency ignorance or naïveté?

(5) To evaluate the scientific merit of Camelot would be a mere exercise at this point because, under the circumstances described above, a Nobel-quality social science research project would have suffered the same fate. Suffice it to say that with all the data we periodically collect in our own country, we are unable to make good predictions about social, political, and economic conditions. In my opinion, the key persons on the Camelot Project were groping and wrestling with shreds and patches of an extremely complex problem. I think that they had fun and were most handsomely paid, but I failed to detect genuine seriousness and conviction that they even knew how to approach the task before them. Perhaps for light relief, the Camelot offices were furnished with a round table (King Arthur's?) and matching chairs.

(6) We cannot ignore the political atmospheres in the countries potentially involved. Any incident—not just a social science research project—could have titillated Havana Radio, left-wingers, and old-fashioned Yanqui or Gringo haters.

I have taken this time to explain briefly some of the background on Project Camelot and its demise so as to permit a better evaluation of the event. In capsule form, Camelot helped make the storm; ludicrousness, incompetence, cynicism, ignorance, and bureaucratic maneuvering for its own sake are excellent ingredients for disaster.

It would indeed be tragic if the aftermath of Camelot prevents anthropologists and others from working with AID, the Peace Corps, and other agencies. But paradoxically, bureaucrats of the kind I described earlier, lacking background or experience in the fields that they control, are now responsible for vast amounts of federal nonmilitary research monies. So, we see that the problem extends beyond military-sponsored research.

Anthropologists may forget too easily that the Bureau of American Ethnology and the Bureau of

Indian Affairs have been responsible for a great deal of anthropological interest and research. Working for the United States Government should not suddenly become sinful. What matters is that the researcher or scholar retains his professional integrity and standards. There should be times when a researcher should refuse an assignment if it means that he must function in a manner contrary to his professional and personal ethnics. A wise government must expect this and would produce an assignment compatible to the man and the mission.

I am sure that most university professors and intellectuals, in and out of government, feel responsibility to their society as well as to their chosen field of endeavor. I doubt that these responsibilities need be contradictory. If they are, our nation is in deep trouble.

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ON THE RELIABILITY OF THE ETHNOGRAPHIC ATLAS

Sir:

Unlike many social scientists in this country, I place considerable value upon the efforts of Murdock and others to continue the cross-cultural tradition associated with the names of Tylor, Wheeler, Hobbhouse, and Ginsberg. It is therefore distressing to see unnecessary errors such as appeared in the recent issue of *Ethnology*.

Under the heading LoWiili, the entries for columns 74 (inheritance of real property) and 76 (inheritance of movable property) appear as follows:

74 Pe
76 no entry

Not only is no entry made under movable property, but an asterisk in table 74 leads the reader to a footnote that states: "Comment. Column 74: The data on inheritance are cloudy and contradictory."

It was four years ago that *Death, Property and the Ancestors* (1962) was published (in America). Although it includes a very detailed study, theoretical and ethnological, of the institutions of property, including inheritance, this book does not appear in the list of references. Even so, in an earlier publication to which the Atlas does refer, namely, *The Social Organisation of the LoWiili* (1956), I discuss the inheritance system in several places as it was already of central concern to my work. On page 21, I state "The LoWiili . . . recognize descent groups based on both lines but property of all kinds is inherited patrilineally." The same information is repeated on page 60, where I also discuss certain earlier moves, in some families, toward uterine inheritance; the two following pages are concerned with the differences between the LoWiili and the LoDagaba in relation to inheritance. On page 108,

I again discuss the fact that the LoWiili transmit "all property patrilineally."

It is quite clear that either the person or persons responsible for categorizing this material did not read with any great attention the sources from which the material is said to be abstracted, or, alternatively, he did not understand what he read. I do not claim that the material I present on these peoples is simple. I quite understand, from personal experience, that in comparative work there is a tendency to prefer a simpler statement about, say, inheritance or marriage rules, based upon the results of a rapid survey or the use of a few informants, to a report of greater complexity, based upon field work of the more intensive kind. Rattray's material on Ag. 43, No. 988, the Vagala (who incidentally are not to be identified with the Siti; nor do they come under an "Abron chief"), seems to have been collected in the space of several days, and the few pages he devotes to this group, from which it is difficult to make much ethnographic sense, draw no such pejorative comment from the editor.

The situation is of more than personal significance. The fact that field work of a few days is given equal weight to reports based upon studies of several years raises serious problems of data control. So, too, does the failure (in most cases) to specify the time to which the data refer and whether the profile offered is based upon a historical reconstruction or contemporary observation (though this information can of course be derived from the sources). With regard to time, the sources on the Adangme of Ghana span the 100 years from Daniell (1856) to Halleran (1951), a period of considerable social ferment and change. With regard to methods, the eighth installment of the Atlas is prefaced by the words: "the Editor, who has worked with two groups of reservation Indians and with one fully functioning society in Oceania, can see no sound reason for differentially estimating the reliability of the findings, provided of course that sound field methods have been employed in both types of situation." I do not query the use of informants for the purposes of reconstructing a "society"; in my opinion many "structuralists" are engaged in this activity more often than they credit. But those who have been involved in collecting and analyzing such data about the past and the present can surely be in little doubt as to which endeavor can provide fuller information. The equation of the two procedures runs counter to the many observations made by social scientists on the process of remembering and on the problems involved in delimiting different normative levels, "ideal" and "actual" behavior, etc.

It would assist those of us interested in promoting cross-cultural research, and in defending the reputation of the Atlas, if the coder responsible for the LoWiili entry could give the reasons that led him (a) to ignore the major source of material, and (b) to exclude repeated statements from the sources he